Cost And Benefits of Near-Earth Object Defenses

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The value of defenses against objects from space is evaluated by comparing the losses expected in their absence to the costs of defenses, which indicates that defenses should be technically and economically feasible for most threatening objects. Impact frequency data are converted into expected losses from objects of given diameters, and that into the marginal benefits of defenses. The marginal cost of search and deflection are estimated and equated to determine the sizes of objects that can be negated effectively. For objects detected long before impact, the analysis determines the optimum sensor size and cost. The programs for long and short period object search and interception have common technological features that make them essentially two phases of a single program.

Introduction

Historical evidence on the frequency and destructiveness of comet and near-Earth object (NEO) impacts can be used to estimate the damage future impacts are likely to produce, which suggests average losses of roughly \$0.5 B/yr, dominated by the largest objects. It is also possible to use data from defense programs and studies of the last few years to make parametric estimates of the cost of search and interception, which are the major components of a defense. The cost of search dominates for small objects and long ranges, while the cost of interception dominates for large objects with short warning. For an object of a given size, it is possible to determine optimal combinations of search and interception that are not overly sensitive to uncertain cost and performance parameters.

The calculations are simplest for long-period comets (LPCs), which are analyzed explicitly. Equating their marginal benefits to the marginal costs of optimized defenses indicates that defenses are cost effective for LPCs or other objects detected on final approach up to ≈ 7 km in diameter. This result, and the technologies required to realize it, are reasonable, relevant, and weakly dependent on model parameters. The maximum size is insensitive to search cost; the maximum diameter is insensitive to interception costs as well.

These results are extended to objects that make many passes through the inner solar system before approaching the Earth. The detection rate is shown to depend primarily on the number of detectors in the focal planes of the search sensors used, whose optimization leads to systems somewhat larger than those currently contemplated. Such detection should be highly cost effective with or without interception capability.

Current levels of technology, if not of integration, could address most threats. For most, search and interception should be not only feasible and affordable, but cost effective. Technology assessments and integration studies could establish the feasibility of defenses within a few years. While only nuclear explosives are generally cost effective with today's technology, non-nuclear concepts could have a wider role, given advances in propulsion and search. Optimal combinations of detection and interception are such that defenses against LPCs could arguably be developed over a period of a decade for expenditures modest compared to expected losses.

Expected losses from NEO impacts

Estimating the value of NEO defenses is a new and still somewhat controversial subject. The first evaluation, presented at the NEO Interception Workshop, was based largely on the information presented there. While the *Proceedings* of that meeting was in preparation, newer data from the Spacewatch NEO search became available, which indicated a much larger contribution from ≈ 5 to 50 m NEOs. Additional Spacewatch data at the University of Arizona and Erice meetings led to further modification of the conventional "Shoemaker curve" shown in Fig. 1. The main feature of interest here is that the curve contains four rough groups of NEOs, which are distinguished by the power law, n, in the dependence of their collision frequency, f, on NEO diameter, D^{-n}. For D < 50 m, n is unknown but ≈ 6. For 50 m < D < 200 m, n ≈ 8/3. For 200 < D < 2 km, n ≈ 2. For D > 2 km, n ≈ 3.

These collision frequencies are converted into expected losses by multiplying f for each D by the loss expected from the impact of a NEO of that diameter. The loss generally takes the form of an area of destruction that depends on the NEO's mass and energy, which can be converted into a fraction of total value distributed over the Earth's surface through a phase-space argument. Integrating over diameters up to D gives the result shown in Fig. 2 (Canavan, 1994). Briefly, NEOs from ≈ 5 to 50 m make a contribution of ≈ \$6M/yr; those from ≈ 50 to 200 m make another contribution of ≈ \$6M/yr; those from ≈ 200 to 2 km make a contribution of ≈ \$100M/yr; and those larger than ≈ 2 km make a contribution of ≈ \$400M/yr. Expected losses are dominated by the infrequent but devastating impact of very large objects, which account for 80% of losses.

Some sensitivities should be noted. The ≈ \$6M/yr contribution from 5 to 50 m NEOs is based on the assumptions that the Spacewatch collision frequencies are no more than an order of magnitude greater than the Shoemaker curve, that only 6% of the small NEOs are metallic, and that the 94% that are stony break up during entry without damage. Should the fraction of metallic NEOs prove larger, their contribution would increase accordingly. If each of these assumptions was in error by a factor of 3, the contribution from small NEOs could approach that of ≈ 200 m group. Conversely, should the number of NEOs in this group—or the damage they produce—be refined downward by such amounts, the losses would decrease to insignificant levels.

The \$6M/yr contribution from 50 to 200 m stony asteroids is from damage on the ground due to shock waves from NEOs that break up tens of kilometers higher (Chyba, et al., 1993). While this has only been an active area of research for a few years, it appears unlikely that the losses from this group could be much greater than those shown, and they could be significantly lower. The \$100M/yr contribution from ≈ 200 m to 2 km NEOs largely represents damage by tsunami generated by asteroids that impact in oceans. Without tsunami, the losses from this group would scale much like those from smaller NEOs and lead to a similar contribution (Canavan, 1994). NEO-produced tsunamis have been studied little (Canavan, 1994). Although it is not clear that the historical record supports even the level of damage estimated, the concentration of population around coastal areas that have become heavily populated in modern times could make the damages much larger than those estimated above.

The losses from large NEOs are only rough bounds derived from the conventional assumption that NEOs larger than 1 to 2 km can cause global damage. There are suggestions that even smaller NEOs could cause global damage, but the arguments for even the larger values are largely qualitative, and not necessarily consistent with the paleontological record. The values shown follow from the following calculation, which both illustrates the method and is useful below. The Earth's gross product G is ≈ \$20T/yr. Thus, if the damage from a several kilometer NEO persisted for a time of T ≈ 20 years, the total loss would be about TG ≈ \$400T. The impact frequency of NEOs with D greater than ≈ 3 km is roughly $f \approx K/D^n$, where from Fig. 1, $n \approx 3$ and $K \approx 10^{-5}$ km^3/yr. The losses from NEOs with diameters between 2 km and D is

$$L = \int_2^D dD\, GT\,(-df/dD) = GT[f(2) - f(D)] = GTK(1/2^3 - 1/D^3). \quad (1)$$

The expected annual loss from NEOs with 2 < D < 3 km is ≈ \$400T x 10^{-5} km^3/yr $(1/2^3 - 1/3^3)$ ≈ \$300M/yr, which is roughly the increment between the "sub-global" < 2 km and "global" 3 km contribution shown in Fig. 2.

Each factors in this estimate is uncertain. The collision frequency of large NEOs is uncertain by about a factor of two. The time persistence of the destruction is uncertain by about an order of magnitude. The expected loss is uncertain by several orders of magnitude. The loss of production is used above to denominate losses, but it is only a surrogate for value losses that are more difficult to quantify. Moreover, these losses are sensitive to the time for preparation available before impact. If a NEO of this size hit without warning or preparation, the devastation could be global and total. If there was adequate warning and preparation, the above estimate of losses should be reasonable. If there were adequate defenses, losses could be minimal. The first case corresponds to living in an uninsured house and hoping there is no fire. The second corresponds to building a spare house as insurance. The third corresponds to protecting the house from fire. The first is simply foolish. Whether the second or third is preferred depends on the costs of the defense relative to the losses estimated above, which is discussed below.

Benefits of defenses

Integrated losses alone do not determine the value of defenses. Deployment decisions are made on the basis of marginal economics. Since the benefits of defenses are measured by the losses they prevent, the value of NEO defenses is determined by the marginal benefits curves of Fig. 3, which are produced by differentiating the integral loss curve of Fig. 2. There are four segments, corresponding to the four groups of NEOs discussed above. The first starts at ≈ \$1M/yr-m at 5 m and falls to ≈ \$10K/yr-m at 50 m. The curves for 50 to 200 and 200 to 2 km NEOs fall from ≈ \$0.1M/yr-m to \$0.01M/yr-m. The curve for large NEOs falls from ≈ \$4M/yr-km at 2 km to ≈ \$10M/yr-km at 8 km. The calculation of the marginal losses of small NEOs is intricate; for large NEOs it is simpler. For them, Eq. (1) gives the integral losses, and its derivative gives the marginal losses due to NEOs of diameter D

$$L' = GT\,(-df/dD) = 3GTK/D^4, \quad (2)$$

which explains the strong slope of the marginal benefit curve and reinforces their sensitivity to uncertainties in the extent and duration of the damage.

Detection

Detection is a significant fraction of the cost of defense. It varies strongly with technology and basing, varying from perhaps a few \$M/yr for search over centuries with existing ground-based telescopes through a few \$10M/yr for search over decades with improved ground-based telescopes to a few \$100M/yr for rapid search with space-based sensors for objects on final approach. The first is underway, productive, and running efficiently with modest funding. This section discusses the scaling of the latter two. Both require adequate signal to noise for detection, and both depend on recent advances in large format charge-couple detector (CCD) focal plane arrays, which provide high sensitivity over wide fields of view (Canavan, et al., 1994). In the next decade it should be possible to build arrays with a few tens of millions of detectors with high quantum efficiency (Q) throughout the visible and near infrared and very low dark currents and readout noises for a few cents per detector (Wood, et al., 1994). Under those conditions the signal, S, required to achieve a given signal-to-noise ratio (SNR) is approximately

$$S_{req} = \sqrt{(SNR^2 B/Qt)}, \tag{3}$$

where t is the exposure time and B is the background, which can be approximated as $B = B''A\theta^2$, where A is the sensor aperture area, θ is its pixel diameter, and the constant $B'' \approx 0.25/m^2\text{-arcsec}^2$ for space-based systems and about 2.5 times that for ground-based systems. The signal received by a sensor of aperture A from a NEO of diameter D at range r near opposition is

$$S_{rec} = JAD^2/r^2(1 + r)^2, \tag{4}$$

where J is a constant. Equating S_{req} and S_{rec} specifies the sensor needed. That is done below, after a discussion of the proper choice of exposure time.

Rapid search

Rapid search is appropriate for either an improved ground-based system or a fast-response space-based system, both of whose detection rates are maximized by choosing an exposure time such that all of the accessible sky is covered once before any area is covered again (Canavan, 1995). For a ground-based system, that requires searching at a rate of $W' \approx 130$ degree^2/hour; for space-based systems, the search rate could be somewhat larger. For a sensor with a field of view (FOV) of w, full coverage in time t means that t must satisfy $w/t = W'$, or $t = w/W'$. The exposure time for rapid wide-area search is quite different than that for earlier NEO searches, which maximized t in order to search to the largest magnitude possible with a given sensor. Wide-area search sacrifices limiting magnitude for broader coverage. More can be said about the details of rapid search (Harris, 1995), but that is all that is needed for this assessment.

Ground-based search

Ground-based signal requirements are determined by substituting $t = w/W'$ into Eq. (3) and equating the result to Eq. (4) to produce

$$S_{req} = \sqrt{(SNR^2 B'' A\theta^2 W'/Qw)}. \tag{5}$$

It would be difficult to improve the SNR ≈ 4 or $Q \approx 0.8$ of current sensors. And θ, which is set by atmospheric seeing, cannot be greatly improved without degrading FOV. Thus, S_{req} essentially scales as $\sqrt{(A/w)}$. However, A and w are not independent. For optics of a given f number, f#, detector pitch, p, and number of detectors in the focal plane, N, the FOV scales with aperture area as $w \approx Np/f\#A$, so that $A/w \propto A^2/N$, so that $S_{req} \propto A/\sqrt{N}$ in terms of the fundamental sensor parameters A and N. Equating this result to S_{rec}, simplifying, and squaring the result produces

$$N \propto [r(1 + r)/D]^4. \tag{6}$$

Note that the aperture area A has canceled out. For rapid search, what matters is the number of detectors. To first order, the cost of the ground-based sensor is proportional to that of its focal plane, which is proportional to the number of detectors N. Thus, the cost of a ground-based search system capable of detecting NEOs of diameter D at range r is proportional to N and hence

$$C_{ground} = G[r(1 + r)/D]^4, \tag{7}$$

where L is a constant. L could be estimated from the parameters in the equations above; however, given the uncertainties in their values it is perhaps as well to estimate L from current systems. Spaceguard sensors intended to search for 1 km NEOs at ≈ 2 AU were intended to have 10 year campaign costs of about $100M (Morrison, 1992), which gives a value of L ≈ $100M/$6^4$ ≈ $0.1 M-km^4/AU^8. The defense GEODSS system appears to give similar costs. This value of L is used to construct the plot of C_{ground} versus r for D = 2 km NEOs shown in Fig. 4. The solid squares are the ground-based detection system costs, which are ≈ $100M at 3 AU and $20B at 6 AU. The former is about the same as that for the 1 km NEOs at 2 AU used to estimate L. The latter provides a rough bound on the ranges that can be used, in that at a nominal discount rate of ≈ 5%/yr, this $20B total expenditure would correspond to an annual expenditure of about 0.05/yr x $20B ≈ $1B/yr, which is more than the expected annual losses to large NEOs. But within this range of 3 to 6 AU, ground-based detection should be able to support affordable defenses.

Space-based search

Spaced-based search has an added degree of freedom because in space, seeing does not limit the pixel size, so that θ can be made much smaller. If the optics scale as diffraction limited, $\theta^2 \approx \lambda^2/A$, $B \approx B''\lambda^2$, and

$$S_{req} = \sqrt{(SNR^2 B''\lambda^2 W'/Qw)} \ \alpha \ \sqrt{(A/N)}. \tag{8}$$

Equating this to S_{rec} produces

$$NA \ \alpha \ [r(1 + r)/D]^4. \tag{9}$$

For space-based systems, A does not cancel out, so both N and A can be varied to maximize range. Costs typically vary as $C_{space} = nN + aA$, so optimization is accomplished by the choice $A = (n/a)N$, which gives $C_{space} = 2nN$, so that $NA \ \alpha \ N^2 \ \alpha \ C_{space}^2$, which produces

$$C_{space} = G'[r(1 + r)/D]^2, \tag{10}$$

where L' is again a constant to be estimated either by aggregating the parameters in the model or from related satellite systems. While there is no directly relevant scaling base, defense experience suggests that satellites of this complexity could be built for a few $100M. Thus, L' is evaluated from the premise that a system for detecting 2 km NEOs at 2 AU could be built for $100M, which gives L' ≈ $100M[2/6]^2 ≈ $10M. That value is used to construct the curve of C_{space} as a function of r for 2 km NEOs shown by the open squares in Fig. 4. From its benchmark $100M at 2 AU, where space search costs about 10 times as much as ground-based search, the curve rises to $1B at 4 AU and then to ≈ $10B at 8 AU. At the latter it is about 1/10th the cost of the ground-based system, which indicates that space-based systems could have application at longer ranges. Detailed studies could refine these costs, but the main observation needed here is that space-based detection appears to have higher costs for shorter range systems, which is compensated by weaker scaling on range that produces lower costs at the longer ranges of interest for large NEOs.

Interception

To make use of the marginal benefit and search cost curves derived above to optimize defenses, it is necessary to have the corresponding marginal costs curves for negation mechanisms and some knowledge of the ranges and warning times for which they are applicable. Figure 5. shows the ability of various interception technologies to negate 10 m to 100 km NEOs given reaction times of months to millennia (Canavan, et al., 1993). The top line, indicated by dark squares, is the deflection capability of nuclear explosives on interceptors with the high specific impulses and thrusts of nuclear rockets, which represents roughly the maximum capability of current technology. Given reaction times of years, this combination could negate NEOs several tens of kilometers across; given centuries, it could negate NEOs several hundreds of kilometers across. The diameter this combination could deflect increases by a factor of ≈ 20 as the reaction time, t, increases by a factor of 10^4; thus, the scaling is roughly $D \propto t^{1/3}$. It also depends on the interaction technology and coupling efficiency, as discussed below.

The second curve shows the capability of nuclear explosives on conventional rockets, which merges with the top curve for reaction times of a few decades or longer, but falls a factor of 2 to 3 below it for times of less than a year. The third curve is for nuclear explosives on conventional rockets using standoff explosions, which wastes energy, but improves the symmetry of energy deposition and reduces the danger of fragmenting or spalling the NEO (Ahrens, et al., 1993). The fourth curve is for kinetic energy payloads on high specific energy rockets. The fifth is for kinetic energy impact with conventional rockets. The bottom curve is for mass drivers—mechanized conveyor belts that throw material found on the NEO's surface and throw it into space to generate recoil—or other low-thrust, high-efficiency deflection technologies of this type (Melosh, et al., 1994). While they have too little thrust to address NEOs larger than ≈ 100 m with less than a few years warning, with a few centuries warning, they could deflect ≈ 10 km NEOs.

The basic scaling can be understood from the requirement that given a time t to react, the defense must give a NEO on a collision course a deflection velocity just large enough to cause it to miss by the Earth by at least its radius. To do so, the interceptor delivers a final payload mass M_f, whose specific energy density, ϕ, depends on the concept. For nuclear explosives, $\phi \approx 2$ Megaton (MT)/tonne ≈ 9×10^{12} Joule/kg. For deflection by the kinetic energy in head-on impacts, the specific energy is the NEO's ≈ $(30 \text{ km/s})^2/2$, which is ≈ 100 times the specific energy of conventional high explosives, but ≈ 10^{-4} times the specific energy of nuclear explosives. The energy release $M_f\phi$ ejects a mass M_e at a velocity v_e, whose recoil imparts an incremental velocity $\Delta v \approx M_e v_e/m$ to a NEO of mass m. Conservation of energy gives the energy imparted to the ejecta as $M_f\phi \approx M_e v_e^2$, so that $\Delta v \approx M_f\phi/m v_e$. For $M_f \approx 10$ tonnes of nuclear explosives and $v_e \approx 100$ m/s, $\Delta v \approx 75$ m/s for a $D \approx 2$ km NEO. That would deflect it by an Earth radius, R_e, if applied at range $r \approx R_e\, v/\Delta v \approx 0.02$ AU, although applying the whole impulse in one explosion could lead to fragmentation.

The displacement depends on when and where this deflection is applied in the NEO's trajectory. The displacement can be written as $\delta \approx k\Delta v t$, where k is a numerical parameter. For deflection many orbits prior to impact, $k \approx 3$-5 is appropriate, but for LPCs and unobserved NEOs, detection occurs on first approach, most of the response time is used for interceptor fly out, and $k \approx 0.1$ is a more appropriate value (Ahrens, et al., 1993). The latter is used for the calculations below, although k is carried as a parameter for discussions of sensitivity. For defense of the whole Earth, it is necessary that $\delta \approx R_e$, which gives

$$M_f \approx R_e m v_e/\phi k t, \tag{11}$$

which produces the dominant scaling of Fig. 1. For a fixed M_f and NEO density, ρ, $D \approx (\phi k t M_f/R_e \rho v_e)^{1/3}$. Thus, all concepts scale basically as $t^{1/3}$, although that is modified by k, which transitions from ≈ 0.1 to ≈ 5 in the interval from a few years to a few decades. Nuclear explosives achieve the largest D because they have the largest ϕ by a factor of ≈ 10^4. Nuclear propulsion increases k by about an order of magnitude at short t, because conventional rockets fly out at 10% of the speed of the NEO, spend 90% of their time in transit, and make use of only 10% of the detection range for deflection. Nuclear rockets can achieve velocities comparable to the NEO's and intercept it midway. The ejection velocity v_e is an important but poorly-defined parameter, which varies from ≈ 0.1 km/s for deeply buried bursts through ≈ 1 km/s for surface bursts to ≈ 10 km/s for standoff explosions. Thus, it is a measure of the efficiency of nuclear coupling, which produces a factor of ≈ $(100)^{1/3} \approx 5$ variation in D. Since the precise value of v_e is not known and varies with the concept and application, the calculations below assume the v_e appropriate for shallowly buried burst and carry v_e as a parameter for sensitivity discussions. Thus, Fig. 5 represents an upper bound on most technologies, a significant extrapolation of propulsion and penetration, and a somewhat optimistic value of coupling efficiency.

In space, mass is directly related to cost, so M_f can be converted into a rough estimate of interceptor cost. An interceptor payload of $M_f \approx 10$ tonnes would require ≈ 30 tonnes into deep space and ≈ 100 tonnes into low-Earth orbit, which is about the limit of what a fully integrated international effort could now produce. Such a booster could cost on the order of \$100M. The upper stage and controls for rendezvous could cost another ≈ \$100M. The nuclear explosive could add ≈ \$100M more. If life-cycle operational costs were roughly equal to the total cost of the booster, payload, and controls; the total cost for the interceptor might be about \$500M, or \$50M/tonne of payload mass. Assuming that these values could be scaled continuously to other masses gives an interception cost of $C_{int} \approx BM_f = BR_e m v_e/\phi k t$, where B ≈ \$50M/tonne. This is of course just the cost for the booster, upper stage, controls, and explosives, but adding a fixed cost for the ground system and control would increase negation and total costs, but would not affect the optimizations in below.

Cost of defense

The dominant variable hardware costs of defense are those for search and negation. The former are bounded by the estimates above for search from ground and space. As the results are not overly sensitive to the details of the detection model or costs, they assume a cost for detection of the form of Eq. (10), i.e, that for space-based search, which appears appropriate for large NEOs and LPCs. The cost for negation is taken to be that for nuclear explosives on conventional boosters, i.e., the nominal $C_{negate} \approx BM_f$ with B ≈ \$50M/tonne. The total cost of defense is the sum of the costs of detection and deflection, which is

$$C = C_{search} + C_{int} \approx A[r(1 + r)/D]^2 + BM_f \approx A[vt(1 + vt)/D]^2 + BR_e m v_e/\phi kt, \tag{12}$$

where detection range r is replaced by detection time t ≈ r/v. Note that A is *not* the sensor aperture, which has already been integrated into it, but a parameter that is A = \$50M-km^2/AU^4. That value, which is about 5 times the value in Eq. (10), is chosen for a certain degree of conservatism and for consistency with earlier work (Canavan, 1994). Figure 6 shows the total cost C for nuclear deflection. For short times and small D the total costs are ≈ \$10M, which is too small for the model to be accurate. For large diameters they rise to ≈ \$100B. For long times, the total cost is dominated by the cost of detection, which increases as t^2, and is largest for small NEOs. At 1 year, for 0.1 km the total costs are ≈ \$1000B; by 10 km they drop to ≈ \$10B.

There is a progression in minimum-cost combinations that increases from a ≈ \$10M system to detect and deflect 0.1 to 0.3 km NEOs with 0.01 year warning, through a ≈ \$100M system that could detect ≈ 1 km NEOs with 0.1 year warning, to a ≈ \$1B system that could detect and deflect ≈ 3 km NEOs with 0.5 year warning. Of greatest interest here is that for a given D, at short times, the costs for deflection dominate, while for long times, those for detection dominate; thus, the total cost exhibits a minimum somewhere in between. For D = 3 km, at t = 0.01 year, C ≈ \$10B. C then falls to ≈ \$300M at ≈ 0.25 year before rising again to ≈ \$10B at 1 year. For D = 1 km, the minimum is ≈ \$50M at t = 0.06 year; for D = 10 km it is ≈ \$5B at ≈ 1 year. The time that produces the minimum total cost of defense can be determined by differentiating Eq. (12) with respect to time and setting the result to zero. The result is shown in Fig. 7 as a function of D and ϕ. For D less than 10 km and the ϕ of nuclear explosives, the optimal detection times are less than a year. For the smaller ϕ of kinetic and other concepts, the times reach a year at much smaller diameters. For nuclear energy densities there is a break in the scaling at about 3 km. For r >> 1 AU, i.e., t >> 1/6 year, the optimum time is approximately

$$t_{opt} \approx (BR_e m v_e D^2/4\phi k A v^4)^{1/5}, \tag{13}$$

which scales as $(mD^2)^{1/5} \propto (D^3D^2)^{1/5} \propto D$, as seen from 3 to 10 km in Fig. 7. It also scales almost inversely on NEO velocity v, which is of interest with respect to defenses against LPCs, whose velocities are generally higher and more variable than NEOs. In this limit the nuclear t_{opt} is relatively insensitive to most other parameters. An exception is advanced interceptors with much higher fly out velocities, which could increase k from ≈ 0.1 to ≈ 3. That would reduce the optimal detection time by about a factor of $30^{1/5} \approx 2$. For a 10 km NEO that would reduce the optimal detection time and range from about 1 year and 6 AU to 1/2 year and 3 AU. When t_{opt} substituted back into Eq. (12), it gives the optimized (minimum) total cost, C_{opt}, which is shown in Fig. 8 as a function of D and ϕ. The costs vary from a few \$10M for nuclear deflection of 100 m NEOs to ≈ \$1T for nonnuclear deflection of 10 km LPCs. For nuclear deflection of 10 km SPCs, the optimal cost is ≈ \$1B. For t_{opt} large,

$$C_{opt} \approx 5(A/D^2)^{1/5}(BR_e m v_e v/4\phi k)^{4/5}, \tag{14}$$

which scales as $D^{-2/5}m^{4/5} \propto D^{-2/5}(D^3)^{4/5} \propto D^2$, which is stronger than the $D^{4/3}$ scaling for smaller D. C_{opt} depends weakly on detection costs through $A^{1/5}$; more strongly on deflection costs through $B^{4/5}$. Thus, 20% of the total cost is devoted to detection and 80% to deflection just due to the scaling of search costs. Total costs are almost linearly sensitive to NEO and coupling parameters. Based on the expected losses of a few \$100/yr given above, it would appear that the costs for kinetic energy deflection are acceptable for LPC diameters up to ≈ 10 km, which covers the bulk of the threat. Kinetic energy deflection would only appear affordable to diameters up to a few hundred meters. Note that C_{opt} scales as $k^{4/5}$; thus, the 30-fold increase in k possible with advanced interceptors could reduce the total cost of intercept by a factor of $30^{4/5} \approx 15$. For a 10 km NEO that would mean a reduction to ≈

\$100M. For the analysis that follows, the result needed is the marginal cost of optimized defenses. C_{opt} can be written as HD^2, where H contains the parameters of Eq. (14) and is roughly \$20M/km^2 according to Fig. 8. The marginal cost is produced by differentiation as $C_{opt}' \approx 2HD$. This gives the total cost of defenses; the equivalent annual expenditure is determined by multiplying it by the appropriate discount rate, i, so the annual marginal expenditure is $iC_{opt}' \approx 2iHD$. For efforts of national importance, the conventional discount rate is i ≈ 5%/yr. For D = 10 km, these operations produce C_{opt} = \$20M/km^2 (10 km)^2 = \$2B, C_{opt}' ≈ 2 x \$20M/km^2 x 10 km ≈ \$400M/km, iC_{opt} = 5%/yr x \$2B = \$100M, and an annual marginal expenditure of iC_{opt}' ≈ 5%/yr x \$400M/km ≈ \$20M/km-yr.

Effectiveness of optimized defenses

Figure 9 adds these marginal costs for defense to the marginal benefits derived earlier. The bottom curve is for nominal costs; the other two are for costs 10 and 100 times higher to illustrate sensitivity. The bottom curve shows that defenses would be highly cost effective for NEOs with diameters up to ≈ 7 km, where the marginal cost and benefit curves cross. Figure 2 shows that defenses good to ≈ 7 km would prevent essentially all of the ≈ \$500M/yr expected damages. From the results of the previous section, such defenses should cost ≈ \$20M/km^2 x (7 km)^2 ≈ \$1B, or ≈ 5%/yr x \$1B ≈ \$50M/yr; thus, their net benefit would be ≈ \$500M/yr - 50M/yr ≈ \$450M. Note that this \$50M/yr expenditure from marginal economics is a small fraction of the amount estimated from average costs and benefits.

It is possible to derive analytically the NEO diameter at which the marginal benefit and cost curves cross. Equation (2) gives the former as $L' = 3GTK/D^4$, where GT ≈ \$400T and K ≈ 10^{-5}km3/yr for large NEOs. The latter is $iC_{opt}' \approx 2iHD$. Equating the two gives $D \approx (3GTK/2iH)^{1/5} \approx 7$ km. Because of the exponent of 1/5th, this combination of parameters would have to change by a factor of 30 to change the crossover diameter by a factor of two.

The curve indicated by closed diamonds shows that defenses with costs 10 times nominal would still be cost effective against essentially all small NEOs and large NEOs up to ≈ 3.5 km in diameter. Figure 2 shows that defenses to 3.5 km would still prevent ≈ \$400M/yr of the \$500M/yr expected damages. Even partial defenses could negate 80% of the threat, and they would negate the most likely portions of it. From the scaling above, such defenses should cost ≈ \$20M/km^2-yr (3.5 km)^2 ≈ \$250M/yr. However, the curve for 100 x nominal costs lies largely above the marginal benefit curves. It does cut the 5 to 50 m NEO curve about midway, indicating that defense might still be cost effective up to about 20 m diameters, but Fig. 2 indicates that would only justify an expenditure of ≈ \$7M/yr, or a total investment of about \$140M. It is difficult to envision a defense based on current technology for that amount.

This discussion can be extended to study sensitivities to the specific energy of the deflection technology used. The costs of optimized defenses scale as $1/\phi^{4/5}$ for D large NEOs. The specific energy of kinetic energy is ≈ 10^{-4} times that of nuclear explosives, so kinetic energy would increase the cost of defenses by a factor of ≈ $(10^4)^{4/5}$ ≈ 1000. That would increase the marginal costs another factor of 10 above the top 100 x cost curve on Fig. 9, which would be prohibitive. In some circumstances it might be appropriate to pay that penalty to avoid nuclear explosives, particularly the defenses could be built for the assumed scaling costs of ≈ \$50M/yr for small NEOs. However, the scaling of defenses at small D, where interceptor overhead is a larger fraction of payload, is much less certain than their scaling at large D. Thus, alternative and advanced concepts could play important roles in detection, interception, and deflection.

Search over extended periods

The discussion of the previous section applied to search for objects detected on final approach. For them, defenses would have to operate in months to years; hence, it is necessary for their sensors to see as far as possible to maximize warning time to minimize interception costs. For objects that pass close enough to the Earth for detection a number of times prior to impact, it is possible to use a more efficient search over an extended period of time, waiting for the NEO to come close to the Earth, and hence to the sensor, to minimize search costs. Even for extended search from the ground, the detection rate is maximized by rapid, wide-area search (Canavan, 1995), the requirements for which are determined by equating S_{rec} from Eq. (4) to S_{req} from Eq. (5) and using $w \propto N/A$ to produce

$$D \propto r(1+r)/N^{1/4}, \quad (15)$$

for the minimum diameter NEO that can be detected at range r by a sensor with N detectors. For extended search, the detection rate R is proportional the product of the search rate w/t = W', and the integral over the volume containing detectable NEOs, which is

$$R \,\alpha\, W' \int dr\, r^2 \,(M/D^m), \tag{16}$$

where M/D^m is the density of detectable NEOs at r, M is a constant, and m ≈ 2 for most NEOs. Substituting from Eq. (15) produces

$$R \,\alpha \int dr\, r^2 M/[\, r(1 + r)/N^{1/4}]^2 \,\alpha\, \sqrt{N}, \tag{17}$$

While it is possible to estimate the constant in this proportionality, it is adequate to use the current estimate that rapid search with N = 4 million detectors (4Mdet) should achieve ≈ 90% completeness in 10 years, which is consistent with $R = g\sqrt{N}$, where the constant is $g \approx 2/(10\ y \times 2\sqrt{Mdet}) \approx 0.1/yr\text{-}\sqrt{Mdet}$. The time rate of increase of the loss is $dL/dt = GTfe^{-Rt}$, where $fe^{-Rt} \approx 10^{-6}/yr \times e^{-g\sqrt{N}t}$ is the collision frequency of all large NEOs that have not been detected as of t, so the cumulative loss after t years is

$$L = GTf(1 - e^{-Rt})/R, \tag{18}$$

which is shown in Fig. 10 as a function of time for N = 1 to 16 Mdet. For N = 1, the curve increases sharply until t ≈ 10 years. It subsequently rolls over, but the $2.5B loss after 10 years is till about 65% of the GTft ≈ $400T x 10^{-6}/yr x 20 yr ≈ $4B that would occur without any search. For larger N the curves level off earlier at a lower values. For N = 4 the curve levels off at ≈ $2B by t = 20, where the curve for constant N is about parallel to the $2B isocontour, so that increasing t would not increase L. For N = 16, L levels off at ≈ $1B, which is only 12% of the loss without search. It becomes parallel to the isocontour by about 10, illustrating that larger N permits shorter searches. The benefit of search is the difference between the loss without and with it, which is

$$U = GTf[t - (1 - e^{-Rt})/R], \tag{19}$$

which is shown in Fig. 11 as a function of t for N = 2 to 64. The benefits are about $1B after 7.5 years for N = 2, 5 years for N = 64. Thereafter, the loss contours show more gradient in t and N, reaching $4.5B for N = 2 and $7B for N = 64 at t = 20 years. This gradient favors large N, for which U increases more rapidly. In all cases, the linear growth of benefits after a decades results from the fact that most threatening objects are found after the first decade, so that the benefits are essentially equal to the losses in the absence of search. The marginal benefits are given by differentiating U, which produces

$$dU/dN = GTf[1/R^2 - (1/R^2 + t/R)e^{-Rt}]g/2\sqrt{N}, \tag{20}$$

which is shown in Fig. 12 as a function N for search durations of t = 5 to 25 years. While there is a significant spread in marginal benefits at N = 2—i.e., $100M for 5 years to 600M/Mdet for 25 years—it decreases to a factor of 2 at N = 10 and 10% at 64 Mdet. All of the curves fall more sharply as N increases, reflecting the diminishing marginal utility for improving on searches that are already more than adequate. At large N the slope approaches -3/2 because the exponential term in Eq. (20) is small, so that $dU/dN \approx GTfg/2R^2\sqrt{N} \,\alpha\, 1/N^{3/2}$.

The cost of ground-based search are roughly bounded by current university programs, the Spaceguard proposal, and the possible Air Force GEODSS program at roughly $100M for a 10 year campaign. That cost should be roughly proportional to the cost of the sensor used, which in turn should be roughly proportional to its number of detectors in it. If detection and warning alone are adequate to avoid most of the loss from a large NEO, the cost of search and hence of defense can be approximated by C ≈ nN, where n ≈ $100M/4Mdet ≈ $25M/Mdet. From Fig. 12, this m is equal to dU/dN at N ≈ 13Mdet for t = 5 and ≈ 20 Mdet for t = 10 to 20 years. For the latter, the cost of an optimal search would be about $25M/Mdet x 20 Mdet ≈ $500M, which is about 5 times the size of the currently proposed program. From Fig. 10, the residual losses from such a search would be ≈ $1M. From Fig. 11 the benefits would be ≈ $3B, so the net benefit would be ≈ $3B - 0.5B = $2.5B.

For an indication of sensitivity, for these conditions a 4 Mdet sensor would cost only $100M, but would only have benefits of $2.2B, for a net benefit of $2.1 B. A 64Mdet sensor would have benefits of ≈ $4B, but would cost $1.6B, for a net benefit of $2.4B. Thus, there is a larger penalty for undersizing the sensor than for oversizing it. The sensitivity to detector cost can also be seen directly. From above, for large N and t, $dU/dN \approx GTf/2gN^{3/2}$; thus, for dC/dN ≈ n, marginal equality gives $N \approx (GTf/2gn)^{2/3}$. That means that as the cost per detector falls, the number of directors increases almost inversely, which means that the total cost of the sensor varies as Nn α $n^{1/3}$, so if the advance of technology continues to decrease the cost per detector rapidly, future search systems will become much more capable but no more expensive.

An important assumption in the analysis above is that warning alone would be enough to avoid catastrophic losses. Given the presence of some auxiliary system to handle objects that are detected only on final approach, that assumption seems reasonable. However, if a modest capability to deflect objects seen many orbits prior to impact is required , its cost can be roughly estimated from the earlier results. The costs for deflection fall rapidly with warning times on the order of the search times discussed above. They are likely to fall to levels determined by overhead, rockets, and externals more than interception technology. Assuming that a interception mission could be mounted for a cost of a few $100M, those costs would not upset the overall assessment of feasibility established above, and would not impact the marginal analysis at all. The costs of defenses would be added to C and should not be explicitly dependent on N. Thus dC/dN would remain at n, so the optimal number of detectors, and hence the cost of search, would not change. The main effect is that the total cost is increased by the cost of the interceptor. But even interception costing $1B would only reduce the net benefit from $2.5B to $1.5B. Thus, the extended search appears highly cost effective with or without defenses.

Relationship of extended and rapid searches and defenses

Defenses against NEOs and objects that can be detected many orbits prior to impact primarily require competent search for periods of a few decades. Some interception capability would be useful, but it should not be difficult or expensive to provide due to the efficiency of deflection long before impact. The search required can be provided by ground-based telescopes, although they should be somewhat more capable than those currently envisioned. Defense against LPCs and other unobserved objects requires rapid search and ready interception, which suggests space-based sensors, and large-scale nuclear deflection capability. On the surface there is not a ready match between extended and rapid searches and the defenses that support them.

On a deeper level there does appear to be. The extended search for NEOs is a transient problem; one to two decades of competent search should reduce expected losses well below those expected from LPCs. Defense against LPCs is a steady-state response to a threat that can never be eliminated. The only possibility is to reduce the losses they produce. For NEOs, improved search would be valuable; for LPCs, it is essential. Thus, there is need for continuing improvement in search to support both these activities, and any improvement in technology or capability, including space-basing, would help both. Since there is no reduction in the marginal utility of more capable search technologies, search is likely to be a key technology whose progressive development should be promoted for objects of any size and period.

Interception is a more complex issue. In the search for NEOs it is hoped that no objects will be found that threaten the Earth, or that if one is found that is threatening, its predicted impact time would be closer to a million years than a decade. But impact times are distributed uniformly, so there is a probability that an object could be found that would require faster response and hence stronger measures. Again there are transient and stead-state problems. There is currently concern about impact during the NEO survey, or on a time scale too short for limited measures, but after a few decades, that concern will merge into the steady-state concern over LPCs, which clearly take stronger measures. Thus, given the credibility of current assessments that LPCs produce large losses and deserve strong defenses, the development of such defenses is inevitable, and the only question is the time scale for developing them. Providing them on the time scale of a decades would also provide improved search for NEOs, a stronger backup to the interception technologies intended for the NEO search, and expected average savings from defenses against LPCs comparable to those expected from defenses against NEOs. Thus, the technologies and time lines for deployments of the two elements of an integrated defense would appear to be complementary in every way.

Summary

There is now rough agreement on the impact frequencies of objects of various sizes, their damage mechanisms, and the expected losses they produce. There is also some agreement on the uncertainties in each, although those uncertainties tend to be greatest for the largest objects, which have the greatest potential for global effects and which tend to dominate the expected losses from impacts of all sizes. Those losses are currently estimated as roughly $0.5 B/yr, but plausible arguments suggest that they could be an order of magnitude larger. They are in any case strongly sensitive to the amount of warning and extent of defenses available.

The decision to deploy defenses should be based on the equality of benefits and costs at the margin. The marginal benefits can be derived from the estimated losses. The dominant costs of defense are those for search and interception. For the long ranges needed for NEOs, space-based sensors appear competitive with ground-based ones, both appear affordable, and the marginal costs of each can be determined with enough accuracy for analysis. Interception costs can also be paramaterized to about the same accuracy. However, optima are much more sensitive to them than to detection costs—depending almost linearly on most of the parameters of the boosters and explosives. The sum of the costs for search and interception have a optimum detection time and range for any object diameter that can be determined analytically, which permits the analytic determination of the marginal costs of interception. Equating that to the marginal cost of search determines the optimal defense and its cost for objects of any size. Nominal loss and cost parameters indicate that defenses should be cost effective for objects up to about 7 km across, a result that is only weakly dependent on model parameters.

For objects that pass close enough to the Earth for detection a number of times prior to impact, it is possible to use a more efficient search over an extended period of time by waiting for the NEO to come close to the Earth, and the sensor, to minimize search costs. Even for extended searches from the ground, the detection rate is maximized by rapid, wide-area search, which to first order depends only on the number of detectors in the sensor. Since the cost of the sensor is also roughly proportional to the number of detectors, that leads to an analytic optimization, which for current detector cost would lead to sensors somewhat larger than those currently envisioned. The optima have benefits of several $B per decade and costs an order of magnitude less; thus, they are highly cost effective and would remain so even if modest deflection capability was added. As detector costs fall, sensors should become larger but cheaper.

Defenses against NEOs and objects that can be detected many orbits prior to impact primarily require competent search for a few decades. Defenses against LPCs and other unobserved objects requires rapid search and ready interception forever. While these requirements differ, there is a fundamental tie between them and the technologies that support them. The extended search for NEOs is a transient problem; a few decades of competent search should greatly reduce expected losses. Defense against LPCs is a steady-state response to a threat that can never be eliminated. For NEOs, improved search would be valuable; for LPCs, it is essential. Thus, there is need for continuing improvement in search to support both activities. Search is a key technology whose progressive development should be promoted for objects of any size and period.

In the search for NEOs it is hoped that no objects will be found that threaten the Earth, but impact times are distributed uniformly, so an object could be found requiring faster and stronger responses. Again there are transient and stead-state problems. After a few decades, the concern that the NEO survey will find a threat will diminish and merge into the steady-state concern over LPCs, which clearly take stronger measures. If current assessments of LPC withstand review, the development of defenses is inevitable, and the only question is the time scale. A time scale of a few decades would provide improved search for NEOs, a stronger backup to the interception technologies intended for the NEO search, and large expected savings. Thus, the technologies and time lines for deployments of the two elements of an integrated defense appear complementary.

On the basis of current technical assessments, it appears that current levels of technology, if not of integration, could address most threats detected either many orbits or only shortly before impact. On the basis of economics estimates, it appears that they could do so cost effectively. On the basis of the logic of the requirements, it appears that the required search programs could grow progressively from current sensor surveys and that the required intercept capability could grow from current space probes through modest long-response capabilities to those required for fast reaction. Moreover, it would appear that the programs for NEO search and LPC search and defense have in common key technological features that make them essentially two phases of a single program. These conclusions should be tested by more technology assessments and integration studies, which could establish the feasibility of defenses within a few years.

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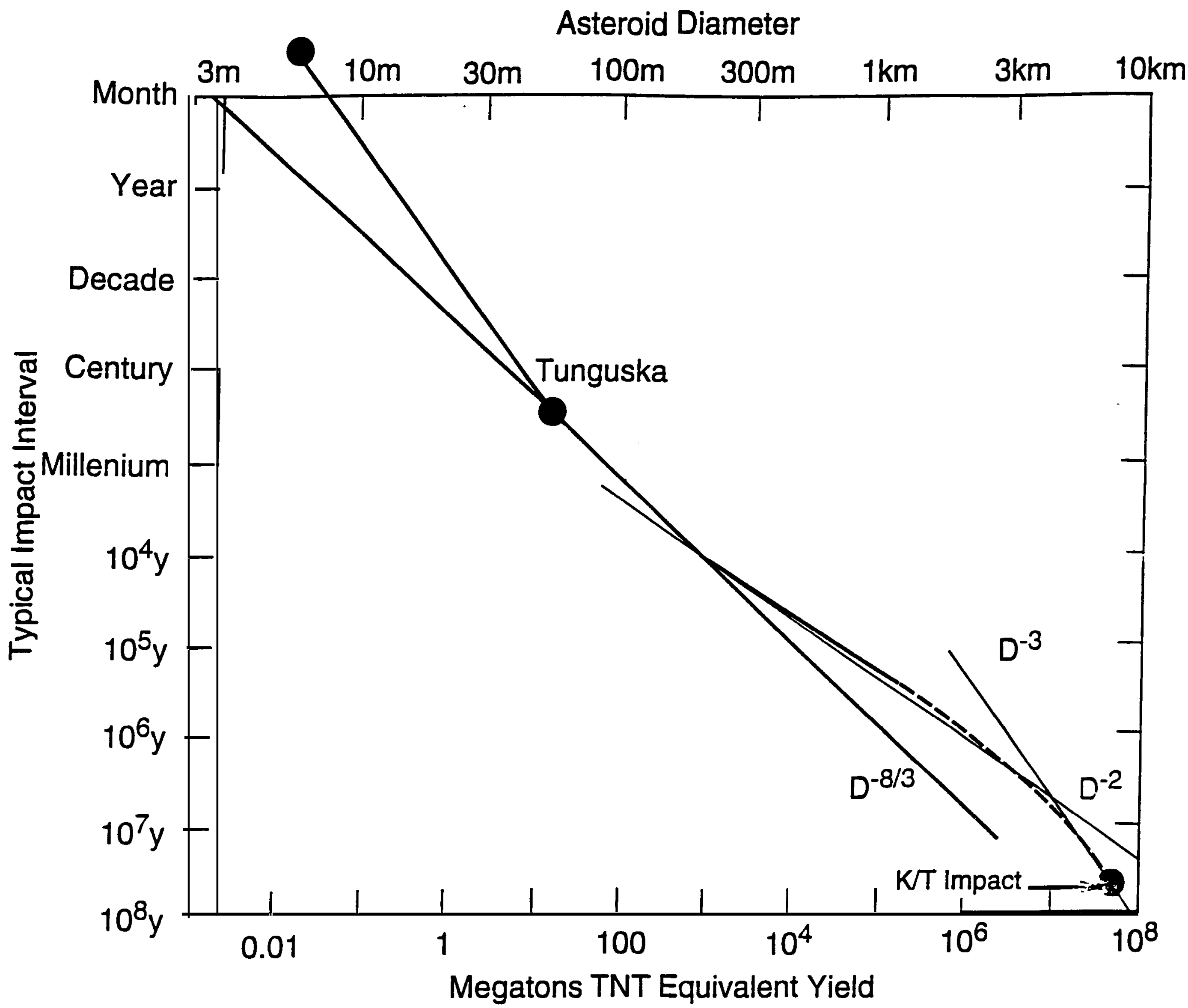
Fig. 1



Asteroid Diameter
3m
10m
30m
100m
300m
1km
3km
10km
Month
Year
Decade
Century
Millenium
10^4y
10^5y
10^6y
10^7y
10^8y
Typical Impact Interval
Tunguska
D^{-3}
$D^{-8/3}$
D^{-2}
K/T Impact
0.01
1
100
10^4
10^6
10^8
Megatons TNT Equivalent Yield

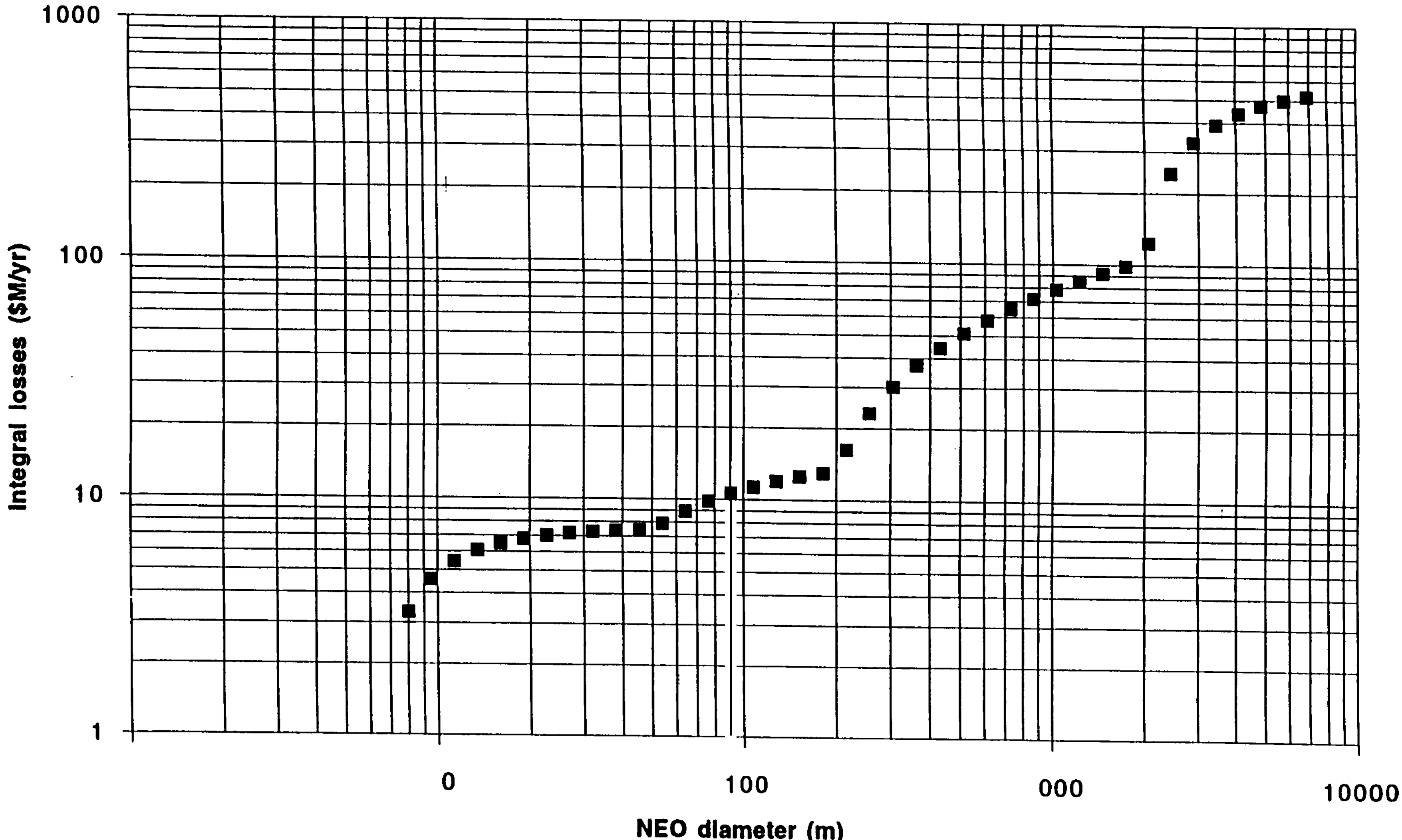


Fig. 2. Losses integrated over diameter.

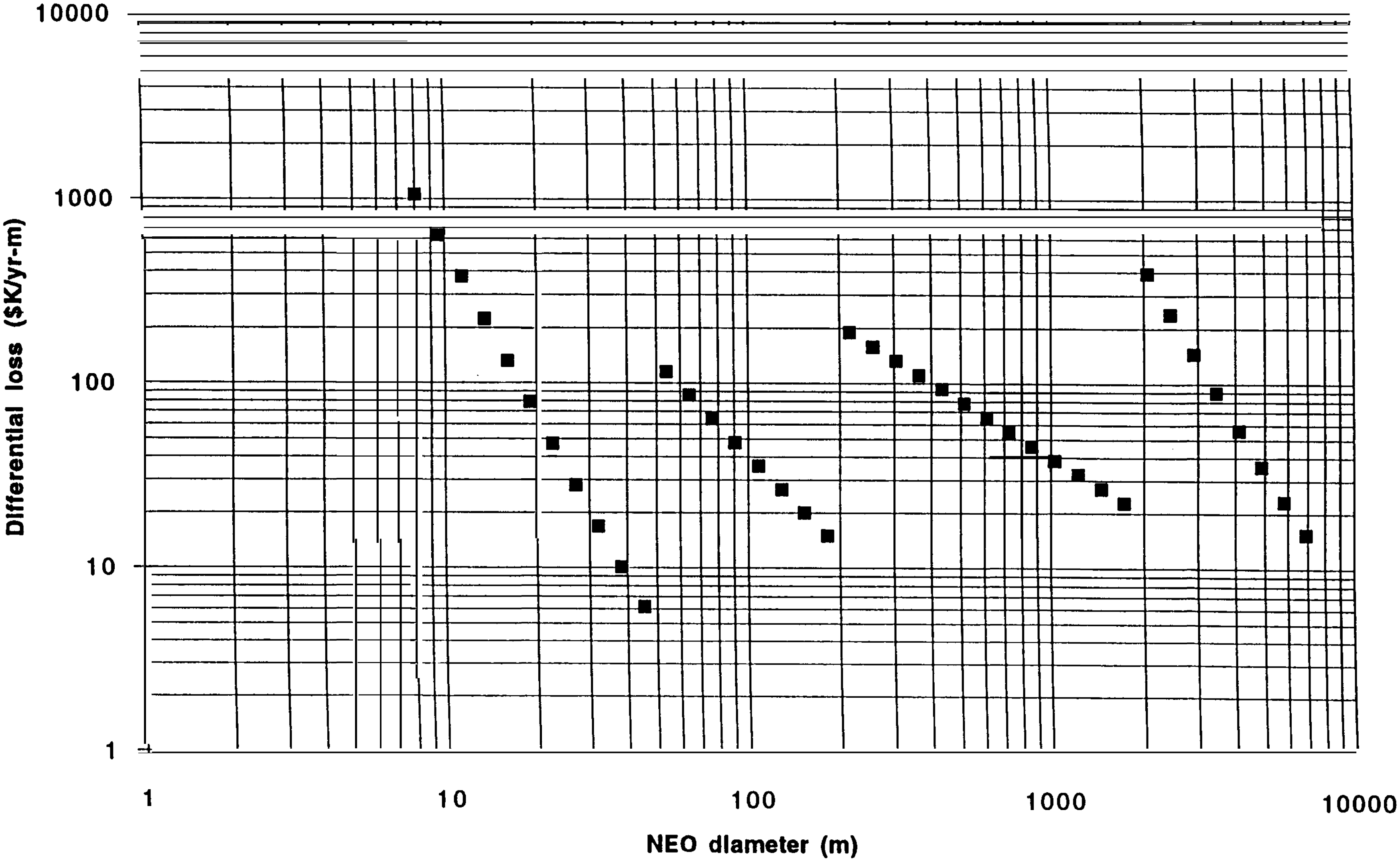


Fig. 3. Differential cost as a function of NEO diameter.

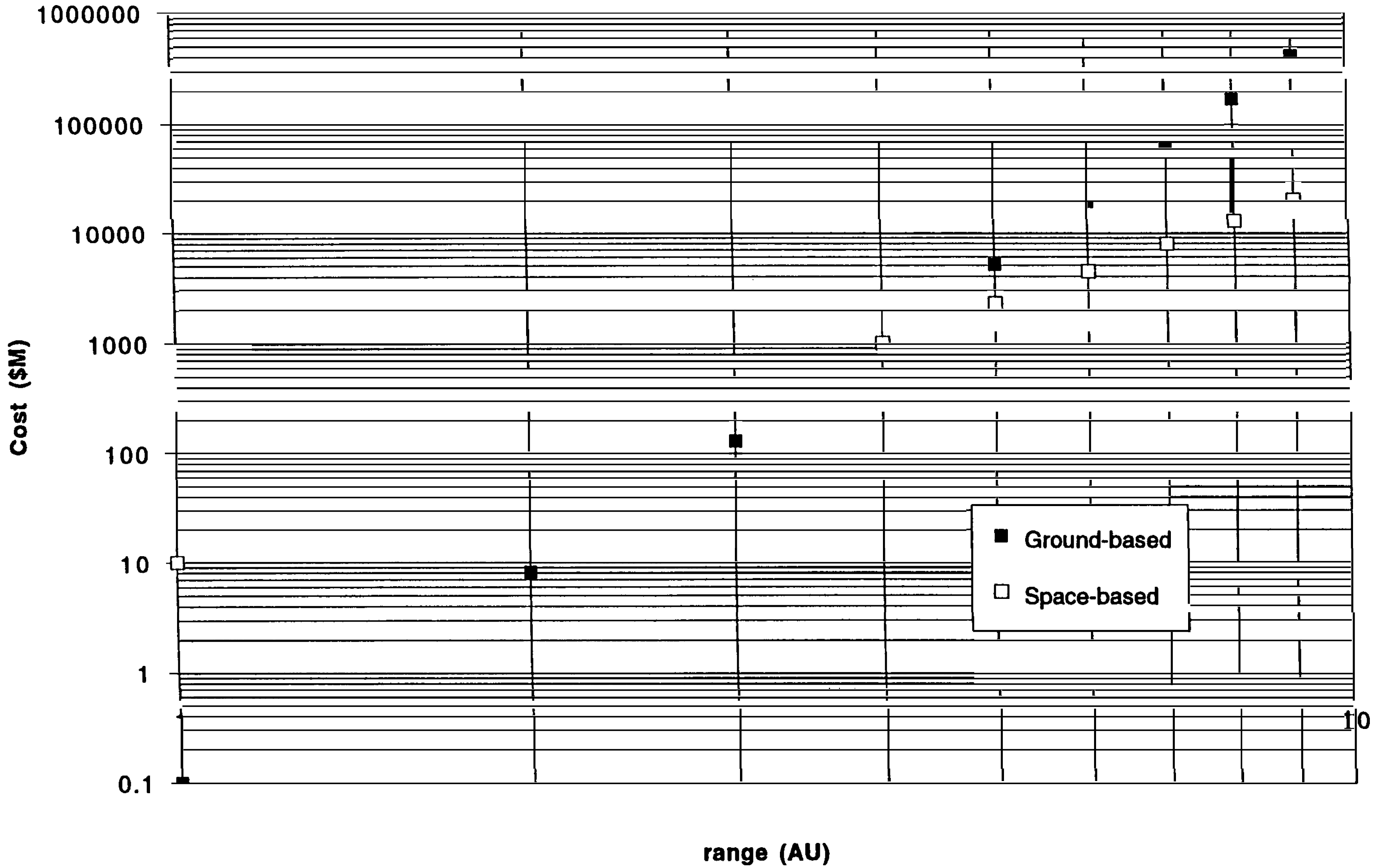


Fig. 4. Cost of ground- and space- based systems for detecting 2 km diameter NEOs at

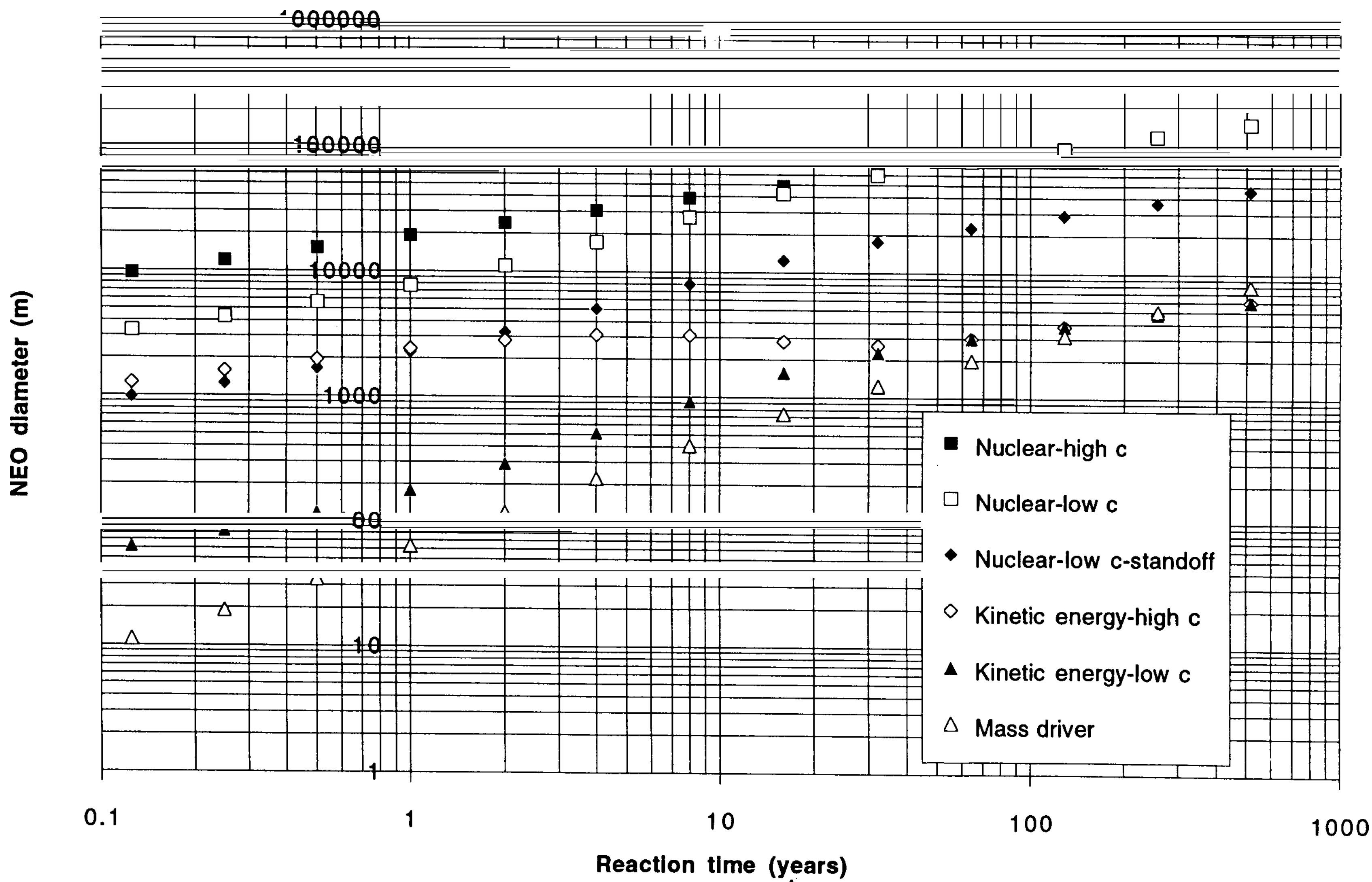


Fig. 5 Maximum NEO diameter that can be deflected as a function of reaction time.

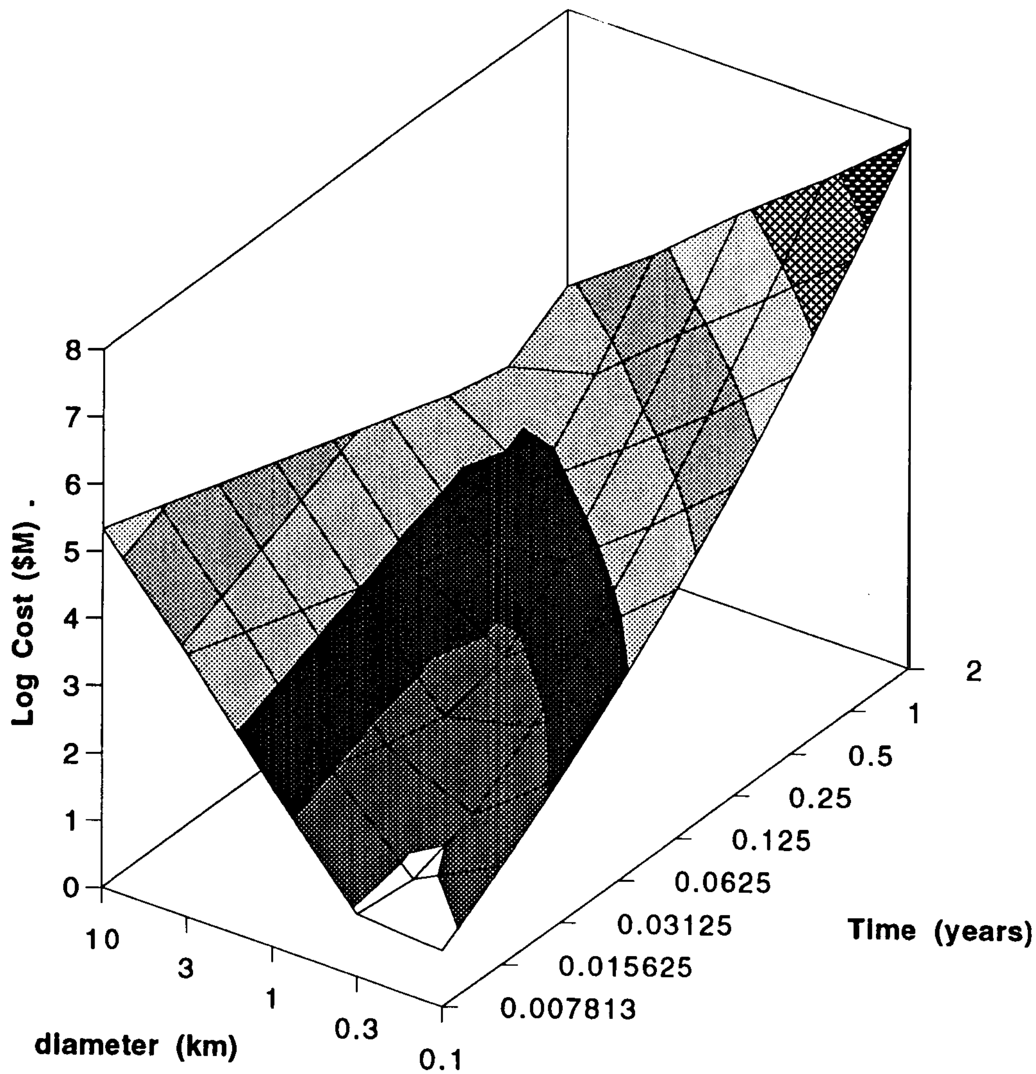


Fig. 6. Search and deflection cost versus time for various NEO diameters.

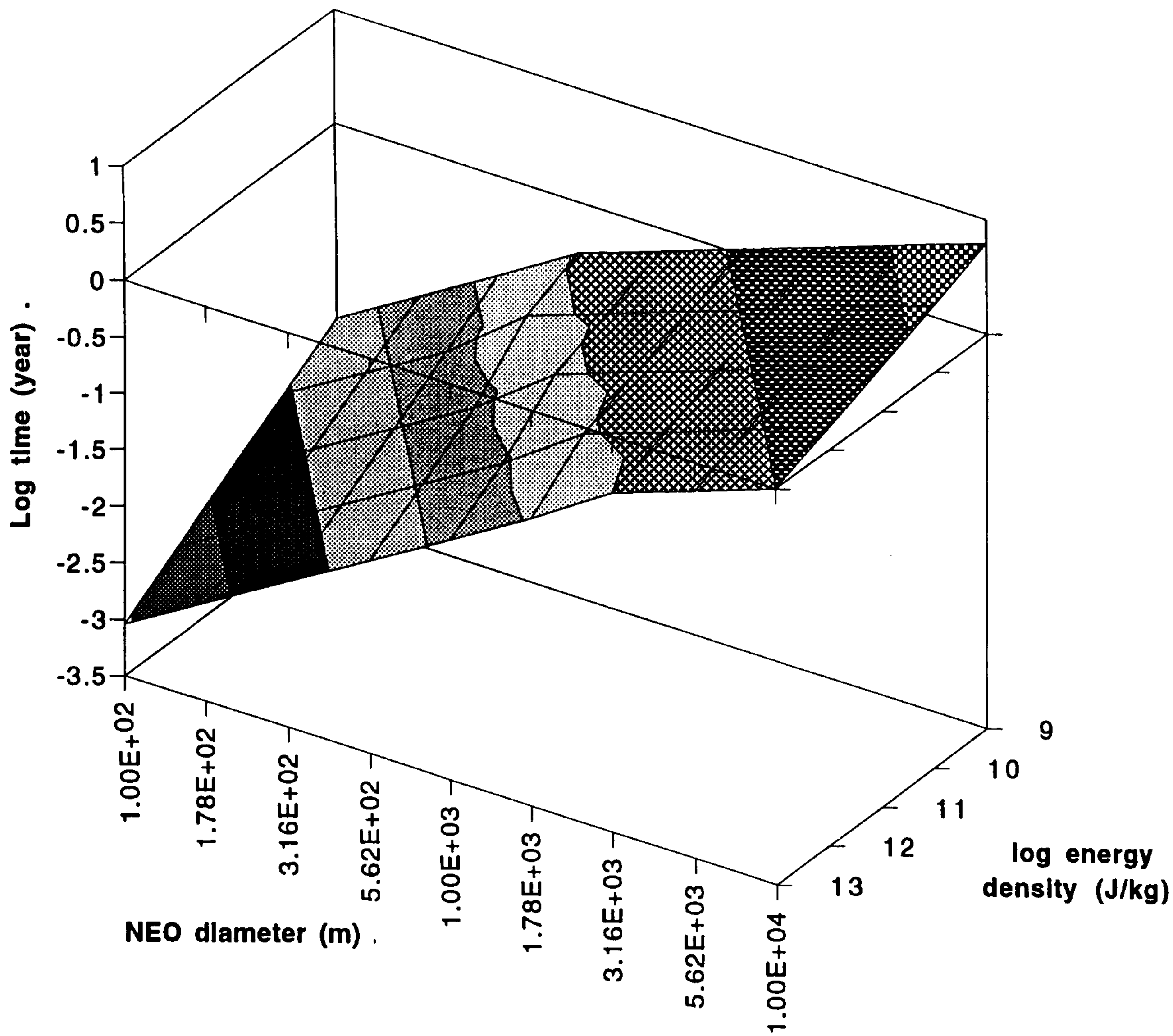


Fig. 7. Optimal detection time as a function of NEO diameter.

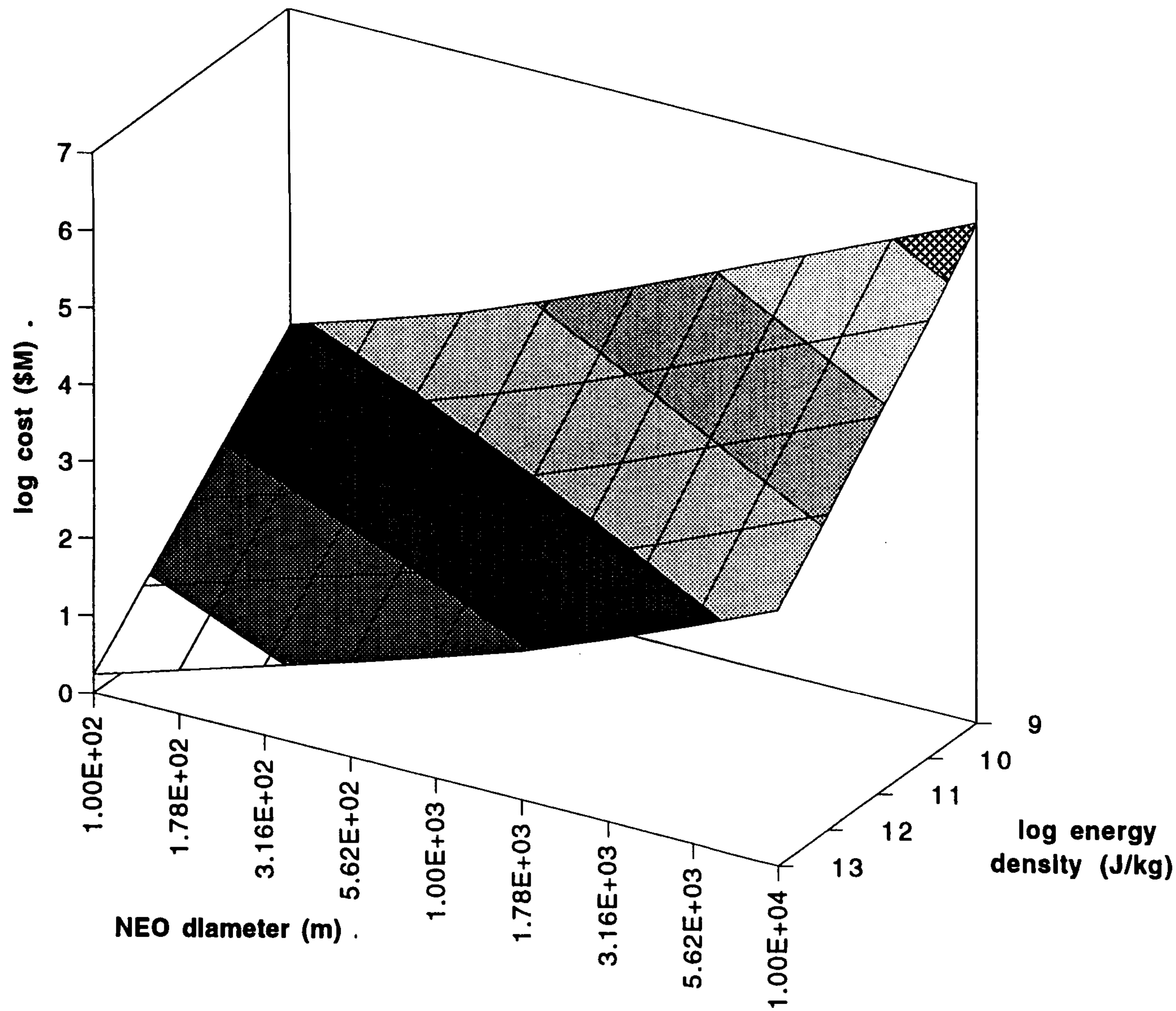


Fig. 8 Cost of optimized defense system as a function of NEO diameter.

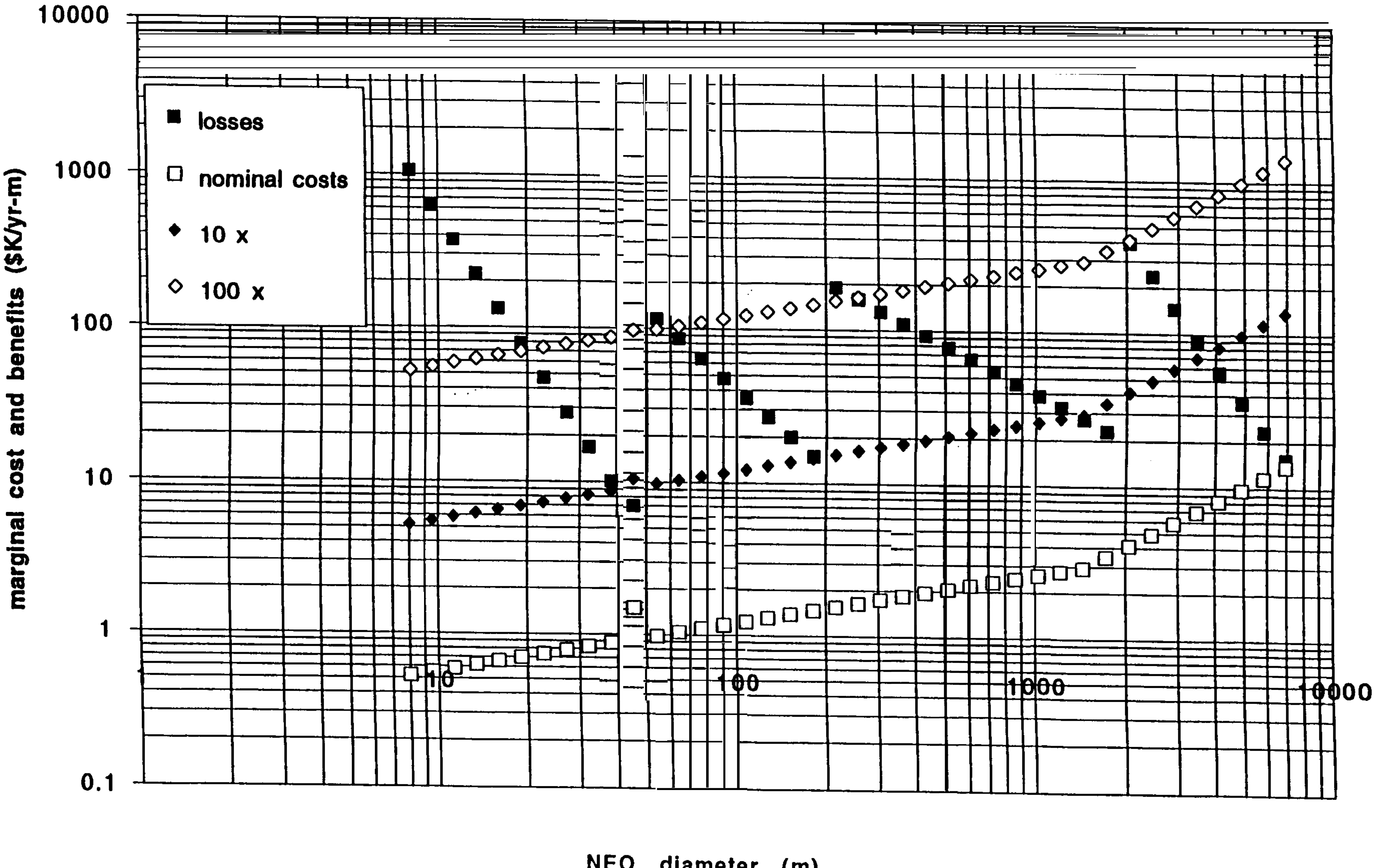


Fig. 9 Marginal costs and benefits of NEO defenses.

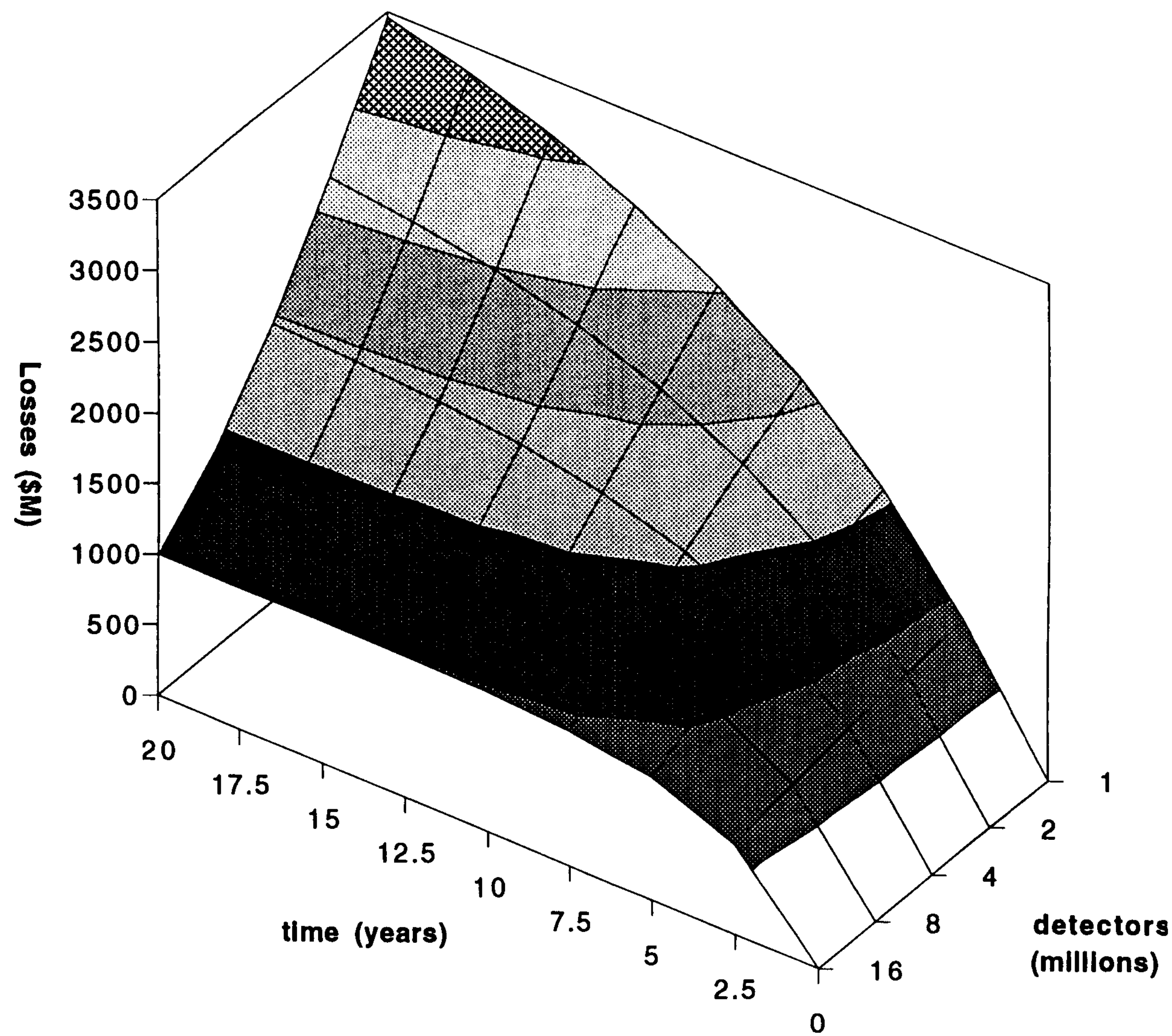


Fig. 0 Losses versus time for various sensors

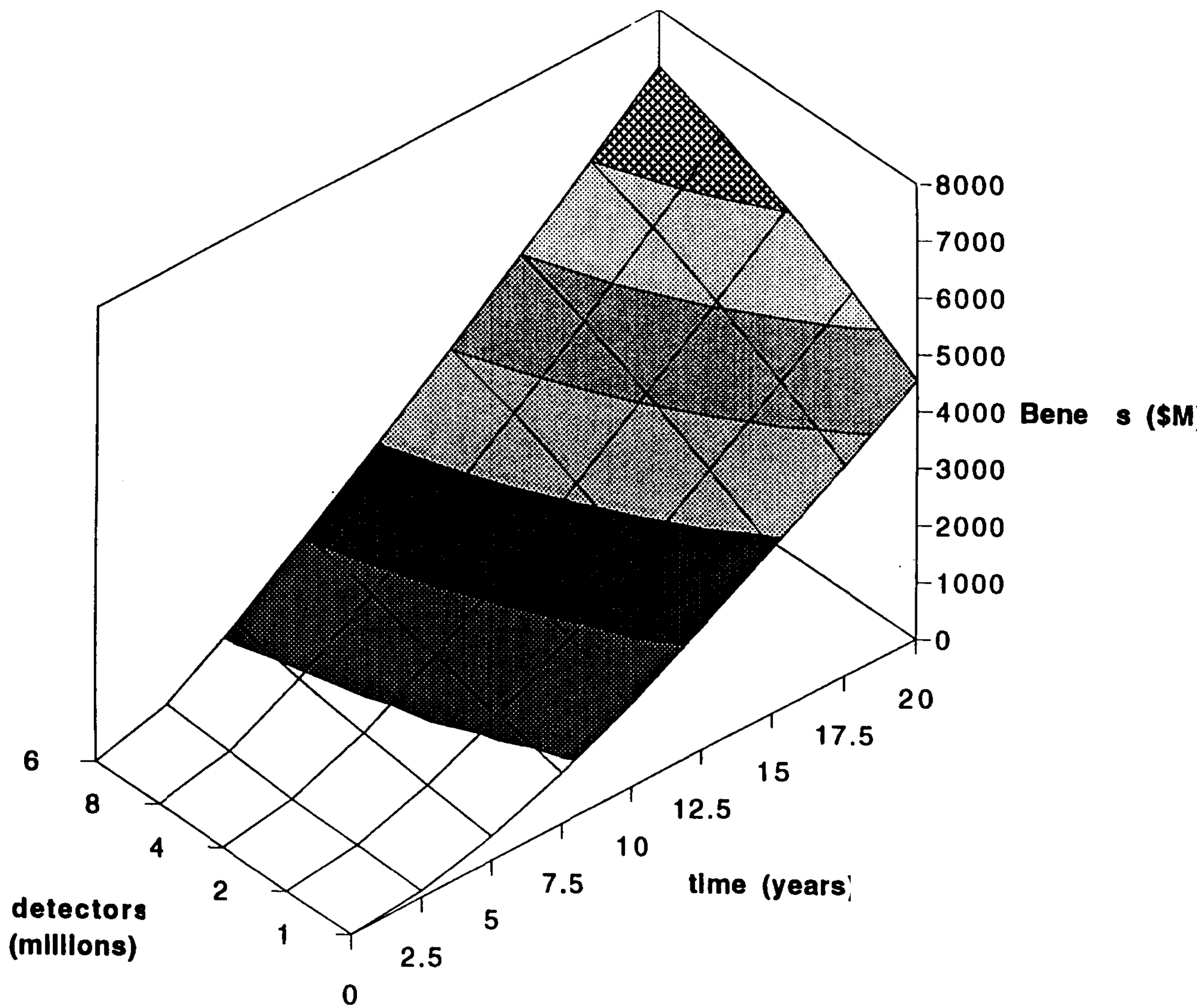
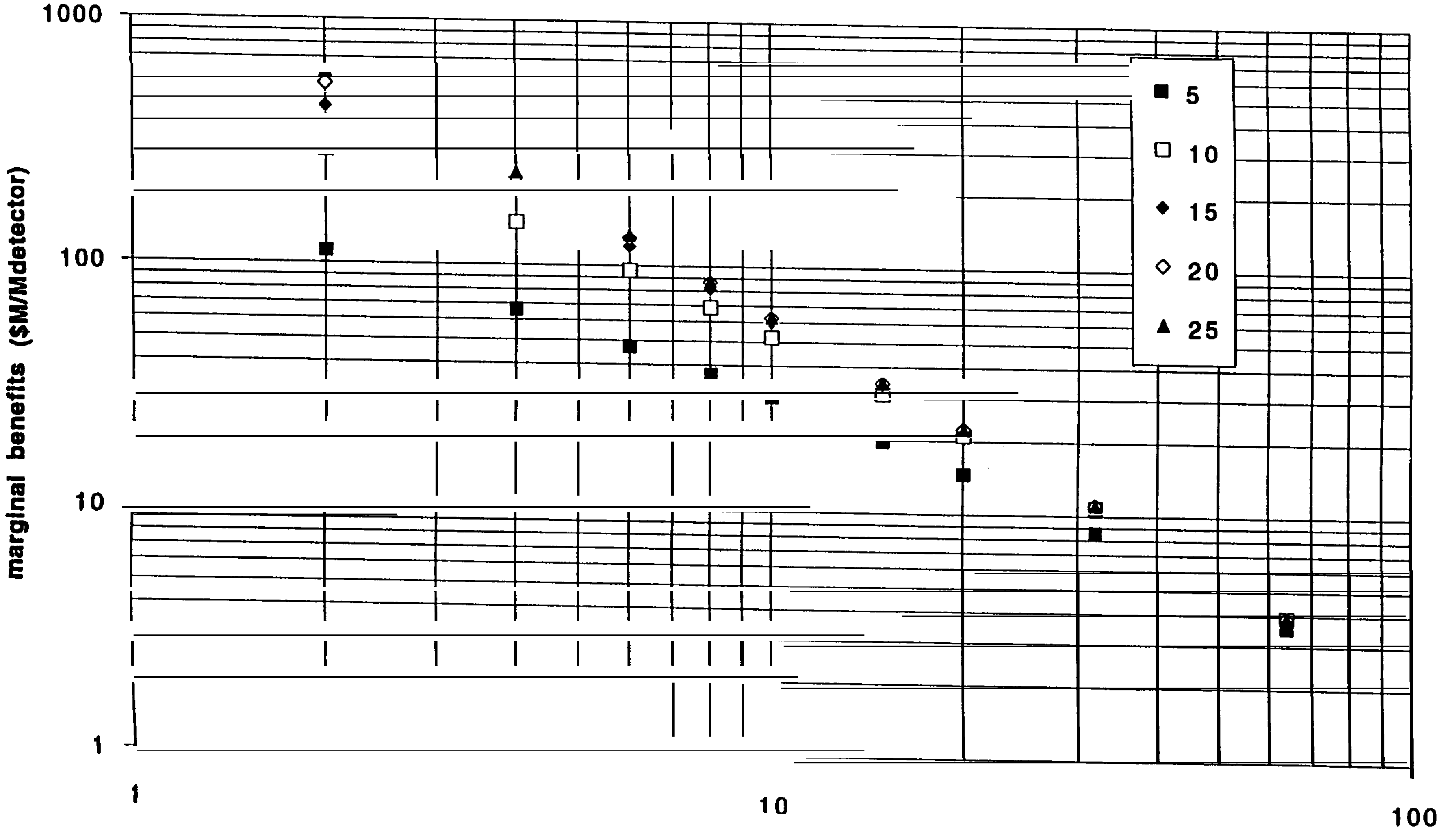


Fig. 11. Benefits as a function of time for various sensors.



Fig

Fig. 12 Marginal costs as a function of the number of detectors and search time.